On some Unapparent Contradictions at the Foundations of Knowledge.

AN argument parallel to that by which Mr. Tolver Preston proposes to prove that Space is nothing will prove with equal cogency that Time is nothing. But if Space is nothing and Time is nothing, then he has the choice of two alternatives, neither of which will he find particularly acceptable. If Space and Time are both nothings, they are identical. If Space and Time are not identical, then they are two nothings which differ. What is the difference between two nothings?

I would suggest that Mr. Preston should read Mr. Herbert Spencer's views on "The Relativity of Knowledge," contained in Chapter IV. of "First Principles." On his carefully thinking this out, and understanding it, I am willing to hope that the title I have adopted for this letter may appear to him appropriate to the subject-matter which he has brought under the consideration of your readers. F. HOWARD COLLINS.

Churchfield, Edgbaston.

Extraordinary Fog in January 1888, at Shirenewton Hall, Chepstow.

THE recent fog has been so remarkable that it seems desirable to record its principal features. From the 7th to the 14th the air was completely saturated with moisture. The most notable feature was that of cold air passing over a warm ground, for from the 11th to the 15th the greatest cold on the grass did not descend to that read at 4 feet. Such a condition of the air as this has not been noticed since I commenced observations in 1838.

The following readings of the thermometers will illustrate this:—

Date.	Temp. 4ft. 10 a.m.	Diff. bet. wet and dry.	Min. 4 ft.	Min. grass.	Diff.
Jan.	°	°	°	°	°
7	43·3	0·0	40·0	34·5	−5·5
8	45·6	0·0	42·1	37·2	−4·9
9	41·2	0·0	37·0	30·3	−6·7
10	34·3	0·0	32·8	28·1	−4·7
11	36·7	0·0	33·3	37·3	+4·0
12	29·8	0·0	29·3	30·5	+1·2
13	28·3	0·0	26·7	28·3	+1·6
14	32·0	0·0	25·0	29·5	+4·5
15	33·9	1·0	28·0	30·0	+2·0
16	30·0	0·5	27·0	27·0	0·0
17	30·5	1·0	29·7	29·7	0·0
18	28·6	0·0	27·0	27·0	0·0
19	31·7	0·4	26·4	24·8	−1·6

Throughout the 12th after 9 a.m. the temperature on the grass was above 32°, whilst it was a frost from the height of 1 foot upwards; at 10 a.m. the temperature on grass was 32°·8, at 4 feet 29°·8, and at 10 feet 28°·6.

The fog lasted from the evening of the 6th till 3 p.m. of the 14th. On the 7th the clouds moved rapidly in W. current, and on the 8th they moved rapidly in S.W. current; on the 9th nearly calm and cloudless overhead; from the 10th to 14th overcast (except from 11 a.m. on the 12th till 12.40 p.m.). The chief direction of the wind was: 8th S.S.W., 9th S.S.E., 10th W.S.W., 11th and 12th calm, and from 13th to 18th between N. and N.E., and on the 19th E.S.E.

The fog was wet and yielded much moisture, viz.:—

7th ·079, 8th ·008, 9th ·015, 10th ·017, 11th ·031, 12th ·013, 13th ·020, 14th ·020, 15th ·023.

The barometer was very high, and almost stationary, reaching a maximum on the 9th at 10h. 30m. a.m., viz. 30·75 inches corrected and reduced to the sea-level.

On the 11th the fog cloud moved in a south current till 3 p.m., when it became north, and continued so throughout the 12th. On this day on the side facing the fog current every leaf and twig had a horizontal deposit of ice, increasing in length from half an inch at 4 feet above the ground to fully an inch at 10 feet; the outside edge of this ice being as thin as the fine edge of a knife; and the whole upper surface of all laurel and other large leaves that were horizontal had a coating of ice, so thin (although it could be detached without breaking) as almost to resemble gold-leaf, on which were transparent impressions of every irregularity, however minute. On the side of trees opposite to this current, instead of rime there were nearly pear-shaped transparent drops of frozen water, of various sizes, mostly as large as one-eighth of an inch in diameter; they were situated *not quite* at the point of every leaf; no leaf was without a frozen drop, and this had an extraordinary appearance, more especially amongst the crowded leaves of such plants as *Pinus insignis*, *Abies Webbiana*, &c. On the opposite side of these fir-trees the appearance was equally singular, as each leaf looked like a knife-blade of one-sixth of an inch in width, with a square apex. The ground-temperature being above 32°, the vivid green of the grass was a great contrast to the ice on the trees. E. J. LOWE.

"The Art of Computation for the Purposes of Science."

IN a paper with the above title, in NATURE, vol. xxxvii. p. 237, Mr. Sydney Lupton refers to some of our work as affording a good example of "the natural tendency of the human mind . . . . to exalt the accuracy of one's own experiments."

The experimental work referred to was a determination by the dynamical method of the vapour pressures of liquid benzene. A curve was drawn to represent these relations; three points were chosen, and the constants for the formula $\log p = a + b\alpha^t$ were calculated. Mr. Lupton finds fault with the number of decimal places given for these constants, and makes three statements which are intended to put the experimental work in as unfavourable a light as possible so as to heighten the contrast with the extreme accuracy of the calculations. Mr. Lupton says: "Nine places of decimals are given with apparent confidence, when (1) only three of the whole number of experiments were made even in duplicate." We do not quite understand this statement, for on reference to the original paper (*Phil. Mag.*, Jan. 1887) it will be seen that the last six experiments in Series I. overlap the first six in Series II., while the last seven of Series II. are within the same limits of temperature as the first four of Series III. The second statement is that "the last pressure, 755, was obtained not by experiment at all, but by extrapolation from a freehand curve, the highest experiment being 79°·6 and 743·1 mm." We would point out that the experiment referred to is not the highest, for on the preceding page in our paper the boiling-point 79°·9 at 753·4 mm. is given. Again, the curve was not drawn by freehand, but by means of engineers' curves, which give very much more accurate results. It is quite true that the last pressure was obtained by extrapolation, but an extrapolation of 0°·1, or even of 0°·4 does not seem very excessive with a range of 80°. Mr. Lupton states, thirdly, that "a difference of $\frac{1}{3}$° at low temperatures produced no change in pressure which was appreciable by the apparatus used." But, as a matter of fact, at 0° a difference of 0°·1 corresponds to a difference of pressure of 0·15 mm., which is quite appreciable on our gauge. Perhaps, however, Mr. Lupton refers to the experiments at 36·15 mm., in which at the same pressure two *different* thermometers registered temperatures which differed by $\frac{1}{3}$°.

Mr. Lupton lastly gives much simpler constants, calculated from our data, and compares the pressure at 60°, calculated from them and from our constants, with the pressure given by Regnault. It happens that the number obtained with the simpler constants exhibits greater concordance with Regnault's value. Now while we would agree with Mr. Lupton in classing Regnault (as far at least as some of his work is concerned) with the select few who are entitled to an extra number of decimal places, yet we would point out that Regnault did not always succeed in obtaining perfectly pure substances to work with, and some of his results are rendered almost valueless on that account. In this case, for instance, the melting-point of Regnault's sample of benzene was 4°·44, whereas after the most careful purification we find that it melts at 5°·58, and the value obtained by Fischer (*Wiedemann's Annalen*, xxviii. 400) is almost exactly the same as ours. Again, Regnault failed to observe the existence of a difference in the vapour pressure of solid and liquid benzene (and other substances) at the same temperature, while this difference has been measured by Fischer by the statical and by ourselves by the dynamical method.

We are quite willing to admit that our decimal points are carried further than is necessary for the calculation of the vapour pressures, but we have frequently had occasion to calculate the values of $\frac{dp}{dt}$ for various substances, and we have found that in order to obtain regular values a large number of decimal places are required; if a smaller number are employed the

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values of $\frac{dp}{dt}$ themselves require smoothing, which involves additional labour.

But if—since vapour pressures only are given in our paper—we have gone to one extreme, we think that Mr. Lupton has gone to the other, for at 79°·9 the pressure calculated from his constants differs by 3·8 mm. (= 0°·16) from that calculated from ours, and by 3·1 mm. (= 0°·13) from our observed pressure, and this difference is certainly too great.

It might also be supposed from Mr. Lupton's constants that the value of b in the formula $\log p = a + b\alpha^t$ could generally be expressed by a very simple number such as the one he gives (– 3·3), but this is not so. It happens that our constant differs only very slightly from the number – 3·3; it is – 3·30062, and by striking off the two last figures in this constant and making a corresponding slight alteration in the value of a, a much greater simplification is possible than would usually be the case.

Mr. Lupton gives five decimal places for log α, and we are unable to appreciate the advantage of using a table of four-figure logarithms where five places are required.

While recognizing the advantage of methods of computation, may we suggest, in conclusion, that, as a rule, only experimentalists are capable of judging the limits of accuracy of experiment, and that they may be trusted to save themselves trouble where trouble may be saved without sacrificing accuracy?

W. RAMSAY.
SYDNEY YOUNG.

"The Mammoth and the Flood."

THE question raised in my previous letter is too important and is being too widely discussed to allow me to let it go by default, and as it has a certain freshness I cannot help thinking that it will prove interesting to many of your readers.

Your critic disposes of Sir Andrew Ramsey in a very unceremonious fashion. To describe the head of the Geological Survey, and the former President of the British Association and the Geological Society, as *an irrational uniformitarian* is to get rid of my attack in a very simple way. Surely some of his scholars or some of his subordinates will have a word to say for their late chief, and, if they cannot maintain his position, will offer some alternative. To the great mass of scientific men who are not geologists, teaching from such a source is naturally accepted as authoritative.

To pass on, however. Your critic speaks of my invoking a series of catastrophes to explain the difficulties surrounding the extinction of the mammoth. This is most inexplicable to me, and points to his not having read my book at all, which was neither fair to you nor me. My book is a perpetual protest against such a series of catastrophes, and an argument in favour of one catastrophe only. May I quote one statement among others?

"If we are to summon some normal cause not now operating for these facts, it certainly seems more reasonable that, with effects so completely alike over such a wide area we should summon *one cause*, and not *several*, and attribute the aberrant conditions showing so much uniformity to some uniform impulse. Here, again, the burden of proof is upon those who deny this view, and treat the remains not as the result of some widespread catastrophe, but as evidence of as many catastrophes as there are skeletons.

"It would be as unreasonable to invoke a separate storm and a separate date for the death of each one of the myriads of razor-bills and guillemots that strewed the western coasts of Britain on a fatal occasion a few years ago, and whose remains were all fresh and in the same condition, as to do the same for the myriads of fresh skeletons of mammoths, rhinoceroses, bisons, &c., in Siberia or in Europe. These debris of a former world have every sign that they formed parts of a contemporaneous fauna destroyed at one time, and are not the wreckage of centuries of deaths."

I now come to what is more important; namely, the theory which your critic resuscitates, after it has been given up by all the Russian inquirers, save one, for many decades—namely, the notion that the mammoths have been floated by the rivers from some undefined land and buried by river action, where they are now found.

Dr. Bunge, who has recently returned from a protracted residence on the Lower Lena, and has described his researches in detail before the St. Petersburg Academy, tells us expressly that mammoth remains are found very seldom indeed in the delta of the Lena, and very seldom also near that river. It is in the higher land separating the great rivers that the remains abound, and especially, as Wrangell and others showed long ago, and as Bunge has recently confirmed, in the mounds and low hills of the tundra. When found near rivers, it is near the short rivers, like those of North-Eastern Siberia, or near the head streams of the Lena, the Yenissei, &c., which could not float such carcasses.

In the next place, Northern Siberia is not a country of mountains and small valleys, but a vast, continuous, nearly level waste covered with moss, called a tundra, diversified by mounds and rounded hillocks, and threaded here and there by rivers running in deep channels—rivers which are frozen fast for a large part of the year.

When the late spring comes, and the ice in the upper reaches melts, while that lower down is still locked fast, there is no doubt a considerable flood in the estuarine parts of the Obi and other rivers, but this is temporary and transient, and it only covers the low lands where mammoth remains are most infrequent. It never covers and cannot cover the higher land. There is not supply of water to do it. To cover the higher points where the mammoth and other remains abound would require such a supply as would put the whole northern part of the continent under water, and thus destroy all animal life there every spring flood. Even if we could postulate river floods of this kind as I have shown, quoting a most experienced geologist, Schmidt, the Siberian rivers deposit no warp that could cover in the mammoths as they are found covered in, by deep beds of clay and gravel, not when lying on their sides only, but when standing upright, as they have several times been found. They must have been covered in by more than two yards of deposit also in a single year in all parts of Siberia, since the ground melts to that depth in the summer, which melting would destroy their soft parts. Appeals to river-floods therefore involve appeals to transcendental causes which are obsolete in other sciences than geology.

Lastly, why is this river portage invoked at all? We have not merely the mammoth carcasses to account for, but the trees found with these great beasts *still rooted*, and the land and freshwater shells showing a different climate when he lived.

Where are we to bring these debris of a former life from? We cannot go outside of Siberia; for the mammoth, so far as we know, has never been found in Asia outside that province. We cannot bring the mammoths found in Kamchatka, and the peninsula of the Chukchi, and the Liachov Islands (which are 150 miles from the mainland), from Central Siberia. Again the remains are very infrequent there compared with their abundance further north, while the mammoths from the north and south of Siberia can be discriminated. There is no sign of rolling on the bones, and the epiphyses are still attached. Evidence of every kind converges therefore upon the conclusion that the mammoths lived and died where their remains are found, and the problem that has to be faced is, how they were exterminated simultaneously from the Obi to Bering Straits, of all ages and sizes, and mixed with various incongruous beasts; how they were buried in the hillocks and high ground under vast, undisturbed, and continuous beds of gravel and clay; and how, lastly, their flesh was subsequently preserved. If all this can be explained without some appeal to the forces I have invoked, then *one factor out of many* in my argument can be answered. If not, it is no use going to Wonderland for hypotheses which only arouse ridicule among students of those sciences which claim induction for their basis. I am most anxious for an answer.

HENRY H. HOWORTH.

Bentcliffe, Eccles.

Is Hail so formed?

I NOTICED here yesterday a curious phenomenon—one that has not before come under my observation.

I was standing under a pine-tree that was laden with moisture from the foggy atmosphere; drops were falling to the ground from the branches, but what struck me was the fact that although most of the drops reached the ground in a liquid state, some of them were converted in their descent into *pellets of ice*.

It was very cold, but I had no reliable means of ascertaining the temperature at the time; it could not, however, have been far off freezing-point.

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